

Madison
March 8, 1953

Dear Spice:

It was a cheer to hear from you and that you had finally gotten home. If you're still interested, I'll mention that the Boston bacteriologists include Dienes, Mueller, Pappenheimer, Paine & Finland, Werner Maas (in Lipmann's lab), and some few others. Or was your lapsus menti entirely unintentional? There are innumerable minutiae, and some important things-- I hope I can keep them all in mind. Bernstein just wrote: from what you said, I judge his coming would be mutually useful. Could you write a formal (not necessarily officious) reference both for my own account, and possibly to help pry funds elsewhere? I'll not say the lab. is empty now-- we have full occupancy of room 200, but no immediate remodelling except to ~~tear~~ down part of the wall between the labyrinth and the central lab. There may be some budget difficulties, but the authorities are talking ~~of~~ tearing apart our entire ~~space~~ space (room 200, 201-central lab-, the two offices, and part of the space beyond the walls, and remodelling from scratch.

Our stay at Chamblee, after a slow start, worked out very well. Edwards warmed up very considerably after we did "make contact", and I look forward to a fruitful, continuing collaboration. (Concerning your typhoid, have you looked for a reaction with M (mucoid)? Felix has all kinds of comments about this sort of thing in his last papers. May we dispose of your residual materials?) Our trip back home was essentially uneventful. We had a fine evening with the K.C. Atwoods at Oak Ridge, then to Columbus, and finally home. There was some snow the last part of it-- enough to threaten, but not too serious. We got back the 18th, and since then my lab desk has supported "a plethora of things adrift", what with the numerous materials we brought back.

Were you about while Edwards and I were discussing the "induction" of phases by some serums, and not others? The story, with Kunzendorf, may give something interesting. Kunzendorf and "berlin" serums, 1,5.... are ineffective, i.e., one gets no swarms or very rough, poorly extending blebs, while other 1,5 and 1,2 serums are effective in evoking c. The first exp't was to mix an effective with ineffective: the result, ineffective, suggesting a selective trouble and more or less disqualifying the idea that ineffective serums lack an incitatory stimulus. The c phases so obtained proved to be inhibited in swarming by ineffective sera, but I had some trouble in demonstrating a corresponding agglutination reaction. It worked in slide agglutination however, so I kept at it, and it turns out that the reactivity of the c phase with berlin ("1,5,....") serum is greatly impaired by treatment with formalin, while c itself is, of course, not. Other c-phases (e.g. SW 902 c-, altendorf --x SW 666) did not react with berlin. The unique factor is also heat-labile. No proof, but strong suggestion that this is another flagellar factor, let us call it temporarily c', until its possible identities are checked. c' is presumably absent from Colindale c-serum, if we accept its presence in kunzendorf itself (by inference from the serum). I have to check with Edwards on whether we should go more deeply into it. To make a more specific serum, we should presumably immunize with cc', living, and absorb with SW-902 or perhaps formalized cholerae suis. The berlin serum which was presumably made with formalized cells, and may not represent the best possible, has a c' titre of over 1:500.

This is quite reminiscent of your remark about b:l relationships. This morning I checked Edwards java strain, N25 and found it agglutinated (liv.) to 1:200 or better in serum from #157, its 1,2:-- derivative, but not at all in Colindale 1,2,3. Is this absorbed? Also, the swarming of N25 is considerably retarded in #157 1,2 serum. I haven't checked formalin effects. Does this mean that a residual antigen may still be present.

I would like to hear more about this story. Is it J.T. working on it? Would you, by the way, make a point of transmitting any part of this letter that may concern her, and with my respects.

The experiments with #157 are about as follows:

abony --x 157 gave	b:--	#157 --x abony:	* 1,2:enx SW938
and	1,2:enx SW926		
LT-2 --x 157	i:--	--x miami	1,2:1,5 (SW973 if
		(just done)	you want it)
SW666 --x 157	b:--	--x N25 (b:-)**	1,2:--
		--x H 901	* 1,2:--
LT-2 (1:1,2) --x 1,2:enx :	1:enx	--x SW-666	* 1,2:--
	(SW-926) 433		

Taken together, these experiments would seem to show that the 1,2 phase of #157 is homologous with typical first phases. * represents types that have been looked for, and not found, with FA from second phase 1,2 in diphasics. The homologies of N25 have not been studied more closely, but ** points to its^b being homologous with H₁.

It is hard to see what this can have to do with the serology, except perhaps in the following terms: the "b" consists of three components, related to b, z₃₃, and in java, to 1,2. The mutation of b to z₃₃, like other artificial phases has been suspected to be essentially a quantitative alteration, and the same might be true of the exceptional #157. If so, these changes and potentialities should be expected to remain together in transduction. It would not be difficult to set up some stocks for comparison: transductions from java to e.g., typhi, and into N25, substituting 1 or some such. Would your colleagues prefer 1) that I wait for their publication before further consideration, 2) that I make up such and other relevant stocks, and send them to Colindale for serological examination, or 3) rely upon your information, and set these up here. I have no preference as between 2) and 3), or any combination, nor would be at all abashed by 1). I have just been starting to study these artificial phases, however, and this information on the cross-reactions is indispensable in the Pratik of the experiments. (For example, I am rather fortunate that to be using Colindale 1,2₃₃ serum rather than 157 serum for selective purposes). Just a shred of suggestion so far, but z₃₃ does seem to be an allele of b, and I have (I hope) a z₃₃:enx.

Transduction in staph sounds a very good idea; I had hoped some year or decade from now to stab at it here. Good luck.

I'd be happy to test the Mushin strain in SRP, if you'd send it with a spot of serum. I append a list of cultures and other material that I'd like to beg from you (and J.T.) Don't be too hard on Hayes; I just fear he's about to fall in with dangerous company. By the way, Skaar's observation that 58-161 may become F- after passage through gelatin-agar has been confirmed, and in another couple of strains too. This must be how 58-161 (Spicer) arose. We're investigating the details; can report only the above exptl. result so far, and whether there's any any connection with motility is unproven.

Kauffmann's book has been "out-of-print"-- could you get me a copy in London? Also Weatherburn, vol. 1. I assume I will have no trouble remitting any cost difference. Thanks very much.

After seeing Edwards' and hearing your and Felix' experience on keeping cultures, I think we'll keep all our material in ~~maxat~~ soft nutrient stabs, and lyophilize only the most critical cultures. In addition, I have deleted "SW-957 to 1033", your misc. Salmonellas, and will keep them under your own label until they are specifically applied. So don't be confused by SW-973, etc. These are my own new numbers. However, I would like to enter "AO 2, AT8, AT14", but have no explicit details on their parentage. Also, I thought you had left a chart of the phage responses of thompsons, dublins, etc. but cannot find same. Can you send me this information, or tell me where to find it, to complete our records?

Not too much more. 1,2:1,5 is probably the last monstrosity we need. IV V XII ~~stern~~ (sendai--x ~~thunyx~~ SW-933) SW-975; IV V XII d:1,2, SW-974, (S. zega --x --:1,2, some other stocks than 157); IX XII lz28:enx, SW-980 (aboy --x jordan) are available if anybody cares.

I wonder if you could ask Joan Taylor if she has any of the following (beyond what is on hand in the stock strains at Chamblee).

S. wien, S. dar-es-salaam, (or any other 1w first and second phase stocks in B-D).

S. ~~wypt~~ napoli -- if numerous strains are available, could you screen them for FIT22 response, if that's less trouble than sending the lot.

S. salinaria

We have either never had, or are ~~wypt~~ running out of certain serums. Those marked * are just for comparisons, and are needed only in minimal quantities. I should be most grateful if you could provide these at your convenience.

b	3
i	1,5
eh	1,6
k	1,7
y	

Bruce sent me a stack of cultures a while ago, but no letter. I'm waiting to hear from him before writing myself-- if you should see him would you tell him I had not gotten any letter ~~as~~ of this date? It may have strayed in the mail.

PS - Anderson